Vol. VII. No. 10. May 12, 1910

THE JOURNAL OF PHILOSOPHY
PSYCHOLOGY AND SCIENTIFIC METHODS

THE RELATIONAL ACCOUNT OF TRUTH

THE problem of the definition of truth is much like that of probability in that both belong to logic and can therefore be settled with a minimum of reference to metaphysics. In obvious recognition of this fact Professor James states that in so far as pragmatism is a methodological theory of truth it is no metaphysic, but, on the contrary, might be held by metaphysicians of whatever stamp.[1] And yet his writings on pragmatism show the constant concern to contrast "the belief that the world is still in the process of making with the belief that there is an eternal edition of it ready-made and complete." Doubtless the metaphysical aspects of truth are worthy of weighty considerations, but since it is confessedly quite unnecessary to thrash out questions of monism and pluralism, of idealism and realism in order to arrive at a logical definition, it will be the explicit aim of this paper to avoid such things wherever possible.

Now if we turn our attention to the problems of truth that seem to call for solution, we find that one difficulty is felt with especial keenness. Truth has often been defined as agreement of an idea with the object, but how this agreement is possible turns up as a perennial epistemological puzzle. It has been explained by Professor Royce as consisting of a form of "one-to-one correspondence" which the idea itself chooses and intends. From a different point of view Professor James regards an idea as agreeing with reality if it "works," if it "helps us to get into satisfactory relations with the rest of our experience." Both assertions are illuminating in their place, and perhaps they are not utterly inconsistent with each other; but however this may be, they bring up questions which we do not need to settle. For it is not solely ideas that can have the character of truth attributed to them. Any kind of a representation at all

[1] "Pragmatism," New York, 1907, p. 54. Compare also the statement, "My account of truth is purely logical and relates to its definition only." This JOURNAL, Vol. V., p. 179.

may be called true; but also many things can be true which are not thought of as representations. The matter that a proposition asserts is not ordinarily regarded as a representation or duplicate of something. Men of science and of practical affairs carry on investigations about the truth of particular matters without at all referring to "ideas" and "objects" and "agreement." Thereby they avoid many epistemological difficulties and differences, and this suggests that we may very profitably do the same.

The way thus cleared we pass on to the further difficulties which are described by Schiller as rendering impossible all definitions of truth not couched in terms of value.[2] Besides the "agreement" definition above discussed, he finds that non-pragmatic literature furnishes only one respectable attempt at a definition. The true has often been said to be that which fits into a system. Schiller's strenuous objections to this formulation seem to be quite sound, but he fails to discern that it contains a certain theme which, we shall later point out, really underlies all theories of truth. He argues with justice that the above formulation is no criterion since falsehoods can themselves form a system. Similarly he adds "It (the above definition) would be adequate if we really had an indefeasible system of absolute truth by whose aid we might detect the inconsistencies of the pseudo-systems. But where shall we find such truth? The bodies of 'truth' which *de facto* we acknowledge in our sciences are all partial systems, incomplete in themselves and discrepant with each other. If nothing short of absolute truth is perfectly systematic, and if all our systems are imperfect, is not all our 'truth' tainted with falsehood, and must it not be admitted that *no* (actual) *systems are 'true'?*"[3] Furthermore, in his well-known way, he insists that it is not good philosophy to conceive individual systems as in the end converging in one absolute and all-embracing system which alone would be indubitably and strictly "true." For this is to commit the fallacy of arguing from the unity of a concept to a similar unity of the concrete ways of exemplifying that concept and so to assume that there is one system and no more into which all truth must finally be fitted. Accordingly he concludes that only two ways remain to fasten down truth: if an *ens realissimum* is not assumed, then there remains nothing else but to tie truth to utility, to particular "fruits" and "consequences."

We desire to submit, on the contrary, that truth can be firmly anchored regardless of metaphysical controversies about the existence of an all-embracing system of relations. This lesson can be abstracted from the results of modern mathematical research. There

[2] "Humanism," London, 1903, Chap. III.

[3] *Ibid.*, p. 48.

is no "absolute" system of spatial relations, no absolute geometry, but within each particular geometry, relations, and the truths that affirm them, are as hard fixed as any "absolute truth" could ever be. We know that the proposition that a straight line is the shortest distance between two points is by no means absolutely true in the sense that Kant took it to be; but with respect to that particular system of relations called the Euclidean geometry it still is considered as *absolutely* true. Certainly relative truth is not absolute truth (truth without respect); but relative truth is *absolutely* relatively true. Look at it from any point you please, and it always remains true with reference to the standpoint to which it *is* relative. If a sum total of particular geometrical systems or of particular scientific systems should be shown to exist as a unitary whole in addition to these particular systems themselves, well and good! We should know something *additional* about being and about truth, but it is emphatically to be maintained that in order to have truth it is not necessary to chain it to the whole of being.

In order to develop this thesis a few words need to be said as to what being *logically* is, as to what is meant by saying that a thing exists. For as Taylor says, and as James agrees, " 'Reality' is, in general, what truths have to take account of."[4] Or to express the same thing in our own terminology, a truth is something that *designates* certain relations which have some attribute or other which entitles them to be called existent. With this recognition of the relation between truth and existence we inquire, What is logically meant by saying that anything exists,—say the traditional fairy, for instance, or Achilles, or the number two, or this desk? Obviously all these things have existence in some sense, since we regard them as legitimate objects for logical discourse. But it is immediately noticeable that some distinction is drawn with reference to the existence of the fairy as compared with that of the desk. It therefore suggests itself first to inquire what is the property common to all things that are said to exist; and secondly, what are the additional properties that belong to such things as this desk which so distinguish them from such things as fairies that the latter come at times to be called unreal?

To say that a fairy has existence is to connect it with that group of facts known to childhood which consists of dragons and heroes and demons and the rest of wonderland. To assert a fairy is to imply these things. Similarly to assert that the number two exists is—as any exact definition of "two" will clearly bring out—to imply the system of whole numbers, or at least some essential part thereof. And finally, when we say that this desk exists, we mean not with reference to fairy-land nor with reference to the whole-number sys-

[4] "Pragmatism," p. 244.

tem, but with reference to such objects as the other things in this room, *e. g.*, this chair, those walls, etc. In short, when one affirms that something exists, one implicitly or explicitly indicates some specific context and says that that thing implies that context. Not to refer to some such context and yet to try to affirm some existence would be to utter meaningless or self-contradictory words. To put this view of existence in a formula we should say that to exist means, with reference to some determinate system of relations, to imply that system.

But since this definition applies to the objects of the reason, the imagination, and the sensible world alike, there immediately arises an objection which needs to be answered before we proceed. For how comes it, then, that such things as fairies have come to be called unreal in comparison with the objects of the physical world? It is simply because we have selected that particular system called the physical world as the one which is most important for our activity and for our thought, and thus in a secondary sense have come to call it *the* real world. But in the primary sense of the term fairies exist no less than do the physical objects.

The first step toward a logical definition of truth consists in making a logical definition of existence. This being done, if we hereupon proceed to say a few words as to what constitutes a discovery that some precise thing exists, we shall find that we have come upon truth almost before we know it. To find out whether a thing exists one of course tries where possible to come in perceptual contact with it. One lets his sense organs play upon it, finds that his sensations do or do not imply the thing, and passes his judgment accordingly. But often it is inconvenient or impossible thus to get at the thing itself, and then one must content himself with examining the thing's environment or such parts of it as are accessible and then one searches in the latter for indications of the presence of the thing. If, for instance, I wish to learn whether "La Tosca" was played at the opera last night, I can not now put myself in immediate perception of the performance, but I can at any rate examine what one might reasonably call the environment of the opera, viz., the play-bills, the newspapers, the knowledge of my friends, etc. If "La Tosca" existed last evening, it existed with reference to a very definite system of relations, and I am now examining that system—or such parts of it as I can get at—to see if it implies "La Tosca." I can be absolutely certain that that system either does or does not imply "La Tosca"; hence all that I should need in order to pass a certain judgment that the performance, let us say, was given, would be to get at and examine all the parts of that system essential for determining the point at issue. But unfortunately I can not get at all those

parts, and precisely because that performance is now an event of the past, and therefore, since I should look in vain for absolute certainty, I have to content myself with reasonable indications that the system implies that "La Tosca" was given last night. In so doing I form the familiar probable judgment. If, on the other hand, not an operatic performance but some abstract mathematical entity were in question, and I needed to find out whether the latter existed with reference to some specified system of mathematical relations, I could (if I had the requisite mathematical ability) make a thorough examination of this mathematical system, and could accordingly pass an absolutely certain judgment that the entity in question, let us say, is implied. But whether one passes probable judgments or certain judgments; whether one is considering the existence of a physical or an imaginary or an abstract logical object with reference to its specified system of relations;—what one does is to search that system for indications that that object is or is not implied. It is easy to put this into a formula at once, and we can illustrate and make it clearer later on. *To be true is, with reference to some determinate system of relations, to designate certain relations which are implied by that system.*

First we will illustrate this with concrete examples; next, show how it meets the difficulties which Schiller and James draw attention to; and this will lead us briefly to investigate in how far it agrees with pragmatic standards, and finally, in how far it must be used—consciously or unconsciously—in any self-consistent philosophy.

The simplest example of truth in the abstract is furnished by the syllogism. The premises state that all A is B and that all B is C; the conclusion, all A is C, is hereupon said to be true, and by this nothing more is meant than that this conclusion designates a relation which those premises imply. Again, the proposition that the sum of the angles of a triangle is equal to two right angles is frequently called true. But of course it is true only with reference to the Euclidean system and not with reference to the pseudo-spherical geometry or certain other non-Euclidean space systems. Euclid's system implies it, while these others do not. To take a physical example, suppose that I affirm that this paper is white. I am justified in believing this statement to be absolutely true provided that I give it any meaning at all. For I mean to indicate a certain complex system of relations in which human beings with sense organs and sensations like my own and also objects capable of affecting those organs like this paper have their place. And with reference to the system of relations that human experience of this sort constitutes, I say that this paper is white, and I mean that this fact is implied by that system of human experience. I do not say that this paper is

white and mean to refer to a system of experience possessed by beings whose color-vision lacks the sensation, white. But I make the statement with precise, though unexpressed, reference to our accepted human experience, and as such the statement is *absolutely* true. In this respect it does not differ one whit from any discovered mathematical fact, and since even James admits that the latter have an absolute and eternal character, the same thing must be admitted of truths which have a more obvious human character.

But the relational definition is very sharply to be distinguished from the description of truth as mere "systematic coherence" which Schiller criticizes with much justice. The Ptolemaic theory, as he would suggest, is quite systematic and yet is held to be false. The real reason for this is that *internal consistency does not make the thing that has it true.* It requires consistency with external things for that, and then the thing is true only with reference to those external things. But in so far as the "coherence" theory suspects that truth is ultimately based on consistency of some form or other, it pleads for what any consistent pragmatism must admit.

But, as I hope has been made clear, a theory of truth based on the implication relation does not have to make reference to an "all-embracing" system of relations. Indeed, even if such a thing as a sum total of relations should be proved self-contradictory, the relational view of truth would be left essentially unaffected. For no one doubts that we have particular systems of relations, and I hope that it is clear that their implications are such as "no further experience will ever alter." What is relationally true to-day can not be relationally false to-morrow if it is essentially the same "what." The Ptolemaic astronomy was no truer a thousand years ago than it is to-day. All will admit that it would be absurd to say that the solar system changed when the heliocentric system came to be enunciated. The latter theory designates certain relations which the solar system implies. The Ptolemaic system does not do this and never did. Hence we may be sure that it is relationally false regardless of whether we shall ever be able to measure it up by an "all-embracing system of reality."

The above suggests the current controversy concerning the mutability of truths, and with this question we will commence a brief discussion of relationalism as compared with pragmatism. James's statement that the true is "only the expedient in the way of our thinking" is succeeded by the remark that the Ptolemaic system was quite expedient before the heliocentric theory was recognized. It would seem to follow that the Ptolemaic theory was once true—else what is the use of speaking about all this? But James frequently draws our full attention by means of some startling paradox, and

when we excitedly turn toward him he proceeds to define his epithets until they come to lose much of the startling character that originally drew our attention. Hence, though he explicitly states that the Ptolemaic theory was once expedient, he is quick to add an explanation. "When new experiences lead to retrospective judgments, using the past tense, what these judgments utter was true, even though no past thinker had been there. We live forwards, a Danish thinker has said, but we understand backwards."[5] Ergo, the Ptolemaic theory was false, after all, and hence it was not *made* false, and likewise the heliocentric theory was not *made* true. If this be but granted, then it would seem possible to resolve some of the entanglement by distinguishing between a truth and the realization or consciousness of that truth. The former in itself would be a mere thing *in posse,* to borrow James's expression for it, while the realization would mark the truth in actual use. The passage from the former to the latter would be the change or mutation. It would be a passage from an abstract thing to a concrete thing which in many respects, at least, should be considered the more important of the two. But though change thus comes in as an important fact, it is confusing to say that it is the truths themselves that change. Ideas change, and thereby acquire new implications. But the implication relation itself is not subject to change, and truth is only a certain aspect of implication relation. Hence truth is like the number two, which it would be useless to call either static or changing.

On one point relationalism and pragmatism seem to take sharp issue. That point is with reference to the greater-less relation as holding between truths. Given, let us say, a set of ten simple propositions and suppose that they were considered with reference to a system of relations by which eight of them were implied while two of them were contradicted. Then if this set were to be compared with another set of ten simple propositions all of which were implied by that system of relations, both relationalism and pragmatism might agree that the latter propositions as a set were *more* true than the former. But now suppose a different situation. Suppose that we were considering two sets of propositions and that in each case every one of them was in thorough logical consistency with the system of relations by which we were measuring them. Then relationalism would unhesitatingly pronounce them both true and could not attribute any meaning to saying that one is *more* true than the other. But pragmatism making truth directly depend upon utility rather than on implication, it follows that wherever one truth is more useful than another it must likewise be more true than that other. And since any given proposition varies

[5] *Ibid.*, p. 223.

in usefulness from day to day it is constantly sliding up and down the scale of truth as well. Now this is untenable. Barring cases such as we abstractly considered above, where they are partially implied and partially not implied by the system that measures their truth; barring these cases, a thing is either implied or it is not implied, it is either true or it is not true, and there's an end of the matter. The proposition, "This particular piece of paper is white," can not be described as less true than one that Taft is president or that I must eat in order to sustain my life, even though these three propositions have various degrees of usefulness.

It is not easy to dissuade oneself that most pragmatists, when they say that the truth of a thing depends on its practical consequences, mean just what is ordinarily meant by practical. What a business man would mean, for instance, if he employed the term. But we have been given plainly to understand that it was never meant to oppose it to the purely cognitive.[6] If we had known that we should never have been so startled! For it ought not to be hard to convince any one that when we think of something as true we have some purpose on hand for which that thing may have value; and in estimating the truth of that thing it may be confessed that we select the system of relations by which we measure it. But no matter how subtly usefulness be defined or redefined, it will always admit of degrees,—provided the term be not subjected to our unrecognizable distortion. And hence whatever meaning "usefulness" may come to acquire, if truth be made a function of it, there will always be some truths less true than other truths because the latter happen to be more useful.

Pragmatists have so frequently challenged their opponents to define their own view of truth that the question came to the writer's mind, "What is it which the non-pragmatists assert, which the pragmatists deny?" The present discussion, however crude its form, serves to hint at the fundamental issue. And yet to what extent the relational account is in utter disaccord with the central meaning of the pragmatists, I should incline rather to ask than to state. I do not see how the essential thesis of relationalism, with whatever particular errors I may have expounded it, can be successfully contradicted by any doctrine. Phenomenalism, for instance, is by no means inconsistent with relationalism. Indeed I hope that it has been shown that distinctly human truths (statements of color, etc.) are quite as absolute in character as are truths that are less obviously human. Relationalism seems to be presupposed—even though unconsciously—in every consistent philosophy. Cer-

[6] *Phil. Rev.*, January, 1908.

tainly it is not so presupposed by a pragmatism that interprets the term, "practical" "in the narrow sense." Mere utility will not inevitably drag along the implication relation. But wherever statements are made that ideas are true because they "work," because they successfully "lead" to other ideas and to other realities,—there we are simply listening to descriptions of the implication relation in certain concrete manifestations. For that one thing can imply another means nothing more than that things go together in our experience; and hence that one thing may "lead" to the other. And without this implication, this togetherness, all value would fade into nothingness.

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ON METHODS AND METHODOLOGY

THE science of methodology has, for many years, been recognized as a branch of logical theory, but, concerning its objects and its methods there appears to be neither general agreement nor clear discussion. It is universally asserted among philosophers that there is a body of knowledge called science, that this has been obtained by a certain vaguely defined scientific method, and that this method should not be ignored in the elucidation of logical theory; but here agreement seems to end. Since the period of Mill and Bain, the results of the study have been meager. The study appears to have become entangled with metaphysical subtleties and to have been barren of definite conclusions which have any recognizable bearing on the sciences from which they have been abstracted. The errors of the old empirical writers have been discovered and pointed out, but no clear and useful conclusions appear to have taken the place of those which have been criticized and abandoned.

This barrenness appears to me to be due to the fact that modern logicians have been largely occupied with the metaphysical foundations of their science, and so have lost touch with the scientific methods which form the rightful subject-matter of their study. The present barrenness and indeterminateness points to the conclusion that a valid and fertile methodology should proceed by the empirical rather than by the metaphysical method.

It is instructive to note that a similar conclusion has been reached by another course of reasoning. This line of thought is best illustrated by some of the views of Professor Dewey. In a criticism of some contemporary logical theory he remarks that "Logical theory will get along as well as reflective practise when it sticks close by

and observes the directions and checks inherent in each successive phase of the evolution of the cycle of experiencing. The problem in general of the validity of the thinking process as distinct from the validity of this or that process arises only when thinking is isolated from its historical position and its material context."[1]

The above striking quotation, which I have removed from its context (and with the context of which I must not be understood to express full agreement) well illustrates the standpoint of the present essay. The value of philosophical controversies is often found in the truths which emerge incidentally rather than in any considerable addition to our knowledge of the topics nominally under discussion. In the present instance, whatever opinions we may hold concerning unsettled logical controversies, the view expressed above is one that well deserves careful thought.

The discussion of the metaphysical nature of judgment and of its relation to reality, which forms so large a part of some modern logical theory, has no doubt considerable value from a certain point of view. But, from the standpoint of the objective philosopher, it is more important to distinguish clearly between good judgment and bad judgment. The degree of abstraction in the more abstruse discussions is too great to be of practical use in assisting us to evaluate the logical processes involved in the actual work of positive science.

Without insisting too dogmatically on this view so far as it relates to pure logic, or attempting to limit the methods which may conceivably be used by theoretical logicians in such inquiries, it will be sufficient for me to point out that whatever cogency such remarks may possess in their relation to logic, is vastly increased when we apply them to methodology.

The process of thought is as old as mankind, and, in its rudiments, is probably a faculty inherited from our primeval ancestry in the remote geologic past long before the species man could be said to exist. Certainly more than traces of the thought process are to be found in the higher animals, and, consequently, a subjective analysis of the thought process will have as its subject-matter a mass of ingrained hereditary material. While reasoning that can truly be called logical is not of such remote antiquity, yet it is universal among mankind. Observers of the most backward savage races or of the very youngest children who have acquired the power of speech are often astonished at the penetration displayed in actual practical

[1] "Studies in Logical Theory," p. 62. The present essay is in no way concerned with the relation of metaphysics and logic or with the theories of Lotze and others which Professor Dewey criticizes; but it strongly maintains the necessity of removing methodology from the metaphysical to the practical side of logical theory.

inferences when the subjects discussed are of such a nature as to form a real part of the experience of the individuals concerned. While children and savages would not express their ideas in syllogistic form, it would be idle to deny the applicability of the term logical to any process of thought by means of which it is possible habitually to advance from valid premises to correct or even to plausible conclusions. Indeed, it can surely be accepted as a fact of history, independent of all theory, that a vast amount of logical thinking in the concrete must precede the development of a science of logic in the abstract worthy of the name.

The application of this conception to methodology is obvious. While reason is universal among mankind and can, therefore, be inferred to have descended from a remote antiquity, that development of the process of human thought which can rightfully be described as scientific investigation is of comparatively recent origin. No doubt the first rough principles are implicit in all correct observation and logical thinking. Germs of the method and some of its practical results can be traced to the dawn of history. The primitive steam-engine of Hero of Alexandria, the universal fame of Archimedes and the principles with which his name is associated are two prominent examples. Yet, there is no evidence that any such investigations had advanced beyond the earlier stages when their development was not great enough to supply material for a special branch of logical theory.

For all practical purposes, we can trace the rise of any considerable body of scientific investigation to the sixteenth and seventeenth centuries. This development having existed during so brief a period, it is evident that there is an enormous antecedent improbability against the assumption that, by a metaphysical method, or by an examination of the thought process as such, it is possible to deduce valid principles of methodology. In this region of thought it is also clear that there is a special danger in too great abstraction. When the process is carried too far, the results are liable to lose any intelligible connection with the particulars from which the abstractions have been made.

Examples of this tendency can be seen when intellectualist logic touches the borders of methodology. One striking instance is seen in the modern treatment of cause and effect, concerning which the conclusion of one school of theorists is concisely expressed by Professor Bosanquet, who identifies this relation with "complete ground."[2]

[2] See "Logic the Morphology of Knowledge," Vol. I., p. 276. "Now it is plain from what has been said that the distinction of cause and effect is self-destructive. It is utterly impossible to be successful in the investigation of a

It is outside the scope of this essay to enter into a full discussion of the idea of cause or of the elimination from it of the time element; but, without entering into metaphysical reasoning, it will well illustrate the standpoint of this thesis to examine its meaning with regard to science. If "complete ground" be regarded from one point of view, it may be interpreted as a very indirect assertion of the ultimate unity of nature and of the principle that no phenomenon or section of phenomena is in reality isolated, but all take their place in the greater cosmic unity. If it means this, it asserts a principle that few, and certainly not I, are concerned to deny, and it is only open to criticism in that the method of stating it is confusing and entangled with much unnecessary controversy. If, on the other hand, it is intended to be of any practical value in science or in methodology it can readily be seen that it fails to accomplish this object.

The scientist, for the purposes of his actual investigations, is by no means concerned with the nature of cause as such (if indeed the degree of abstraction in such a conception is not so great as to have taken from the term all concrete meaning), but of the interrelation of particular causes and particular effects.

It is the essence of the scientist's conception of cause that it is not complete ground, that it is possible to repeat in time certain conditions which we call causes and obtain thereby certain other conditions which we call effects. A scientist finds by experiments that one specimen of nitrogen which he obtains from the air is heavier than another which he obtains from nitrites. The cause of this is not "complete ground" (which is equivalent to saying that the experiment can not be repeated), but the presence of argon in the air. After a long period of careful research, this particular "cause" of the observed effect is discovered. It is in the unraveling of particular conditions such as these that the phenomenal advance of science has come about, and it is only in so far as methodology can throw light on the character of such interrelations that its treatment of cause or of any other scientific conception has any meaning or value in connection with the sciences from which it has been abstracted.

causal relation without reducing it to the intelligible unity of a complete ground." It is obvious in a case of this kind that immediately the relation becomes reciprocal (complete ground) it becomes identical and we obtain the tautology air = air or argon = argon. And so long as we retain the incomplete relation heavy nitrogen we do not necessarily imply argon. Any event that can be repeated in time is necessarily incomplete ground or "concrete events or groups of incomplete relations" (*ib.*, p. 268), and if we advance beyond this to Professor Bosanquet's intelligible unity we reach a sphere where scientific investigation has no meaning. The metaphysical unity we then obtain is an assumption which antecedes the whole investigation. In short, such a treatment of cause has no bearing on scientific work.

The kind of methodology which we may call abstract maintains its relation to the metaphysics from which it has been obtained, but it has lost its connection with science. Such methods as these do not assist scientific investigation, they only occasion confusion. To this kind of investigation the scientist gives a very effective reply by ignoring it altogether, and the quality of his work does not appear to suffer in consequence. Whether or no the metaphysical treatment of methodology be theoretically possible, it seems not unlikely that other lines of treatment may give more practical results. Even if in a remote future it be possible to carry the process of abstraction to a higher degree, yet, for the present, we require a more careful and detailed study of methods in particular and a wider area of data on which to build our generalizations.

The conclusion of this line of argument obviously points to the desirability of reverting to a more empirical treatment, and to the methods of the great founders of inductive logic and scientific methodology. While some of the special ideas of Mill and the other empiricists are open to criticism in detail, there can be no doubt that in their close adherence to actual instances of scientific investigation, in their abstraction of methods from real researches in which they had been used, they discovered an invaluable instrument of research. In the use of this instrument, modern methodologists must not shirk the task of criticizing, and where necessary criticizing adversely, the methods used by the scientist and the manner in which he applies them. Here is a difference between the standpoint of the early methodologist and that which should be adopted by his successors.

In the early stages of the science it was necessary to find some universal starting-point and this was obtained by the analytical examination of processes of investigation which were clear and straightforward. But, after the first results have been gleaned in this manner, there is a danger that further conclusions concerning method, drawn from researches where there is no possibility of difference concerning methods or results, should be of the nature of truisms. No doubt there are still interesting problems of methodology implicit in the analysis of researches on the formation of dew or in the theories of induced electricity, but such do not lie on the surface. At any rate, there should be possible a new kind of methodological investigation which maintains an even closer relation between theory and practise. Such an investigation will endeavor to distinguish between valid and invalid methods in their application to actual problems, and, from instances of their use and misuse, to exhibit the main principles which underlie them.

Some attempts have already been made by philosophers to accom-

plish these more practical aims. The recognized distinction between the individual and the statistical methods is a case in point. Owing to philosophic criticism, no one, outside the sphere of popular journalism, would maintain that so many people must commit suicide in London or New York every year. Such a work as the second volume of Sigwart's "Logik" contains many cogent criticisms of the valid and the invalid application of scientific method. But, as a rule, such investigations are somewhat remote from the actual theories of the contemporary scientist. Standard works tend too much to assume the form of a dictionary of theory. They are ignored by the scientist, who most needs their help, and by the philosopher they are studied without sufficient regard to concrete application.

An attempt to establish a closer relation between theory and practise will be found in my own "Principles of Applied Mathematics," an essay which originated entirely from a study of the problem of secular cooling. That any scientist of note, much less the great Lord Kelvin, should have put forward as probable fact, a theory of geologic time which contained so many patent fallacies, appeared to me so strange that I endeavored to discover the reason. It was soon apparent that the mathematical physicist was hypnotized by the form of his mathematical instrument and that he failed to inquire where it was and where it was not applicable. My own article was an attempt to place this matter on a sound theoretical footing.

This, I am sure, is not the only case where such investigations are possible, and I sincerely hope that similar work will be undertaken by other philosophic writers. The scientist will then find that he can not ignore the philosopher, and the philosopher will discover that he has a rightful sphere a little nearer earth than he has been accustomed to look. Wherever methodology may end, it is here that it should begin.

For such work as this, the rise of modern specialism offers many opportunities. In recent scientific investigation, the range of the individual worker has tended to become so narrow, and his outlook over the main field of science so limited, that errors are continually liable to occur in the application of the results of one branch of knowledge to another. The mathematician dogmatizes over the whole field of cosmological speculation. The biologist makes unproved and far-reaching assertions concerning the methods of evolution which affect our ideas on the nature of man and of society. In all branches of science and in all fields of knowledge this is the age of the specialist.

In the careful examination of the methods of the special sci-

ences, in the determination of their nature and of their necessary limits will be found a large and fertile field for the investigation of the methodologist.

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CONTINGENCY IN AN INFINITE WORLD

IN the number of this JOURNAL for February 3, 1910, Mr. W. H. Kilpatrick champions the thesis that "if the actual universe be conceived as quantitatively infinite," definite and final determination "antecedent to the act of eventuation" is excluded. The event on arrival is, indeed, the inevitable outcome of the actual conditions, but in an infinite universe it can never be inevitable—that is, predictable by an adequate intelligence—before it arrives. *A priori* it is necessarily contingent, since the most perfect deduction is necessarily made subject to the proviso that the conditions remain unchanged. This is what can never be guaranteed in an infinite world, a quantitative infinity offering endless possibilities of new factors appearing in the field of even the most perfect calculation.

The question raised is evidently purely speculative. All actual human prediction is, of course, contingent; but Laplace and his followers have been wont to maintain that for a conceivable superhuman intelligence the future, and indeed all time to its smallest details, would be an open book; or, better, a mathematical formula capable of making infallible disclosures without limit in the past and the future. Mr. Kilpatrick denies this speculative possibility on the ground that it requires the postulation of a closed and finite universe, which is "substantially different from what science assumes our actual universe to be."

But in what sense does science assume the universe to be infinite? Mr. Kilpatrick indicates the correct answer when he explains that he uses "the word infinite . . . in the mathematical sense, to indicate, that is, such a variable number as may surpass any arbitrarily assigned number, however great." At this point difficulty appears, for such an infinite is essentially a positive and not a negative term; it is in itself a perfectly definite quantity. However much it may transcend human grasp and be "infinite" relative to human capacity, it is for the superhuman calculator of Laplace a finite quantity; and it does not appear why such an intelligence should not deal with it successfully, and, if not otherwise hindered, compute with entire accuracy and in utmost detail the whole course of events. Nor is any true infinite regress involved in the consideration of the

new factors furnishable from the stores of a universe infinite in the mathematical sense. The regress would be limited to the extent of the "infinite" in question, which the imaginary calculator would doubtless find little difficulty in exhausting.

Even were Mr. Kilpatrick to cut loose from the support of physical science, and postulate a universe truly infinite, in the metaphysical sense, it does not seem evident that sure prediction by superhuman intelligence would necessarily be excluded, for the organization of the universe might be of a serial character, consisting of an endless repetition of organized systems each of which had relations indeed with its neighbors, but relations of a purely compensatory nature, so that in the course of a given cycle each received from its environment impulses precisely equivalent to what it gave and no more. In that case even in a universe metaphysically infinite, one fails to see why the determination within each system might not be so complete that perfect prediction on the part of an adequate intelligence should be possible.

Suppose, however, we concede to our author the existence of a truly infinite world with universal articulations, what is the practical bearing of his conclusion? Is it more than the overthrow of an incidental speculation? It does not seem to apply to the matters usually at issue in the determinist controversy. For example, the cosmic contingency for which he pleads offers no word of promise to the principle of individuality as this faces the hostile forces of wholesale mechanism. The human individual is immediately known to us as a seat of efficiency, or immanent activity. Determinism, however, goes behind the returns, and resolves this fundamental content of consciousness—which to mature men is perhaps the thing of supreme worth yielded by experience—into illusion, substituting for the perceptual datum the concept of a moving plexus of cosmic agencies, with our consciousness of self as a mere epiphenomenon, the phantasmally continuing rainbow over the stream of mechanical process.[1] Now, just this and nothing more the human individual seems to remain, so far as any bearing of our author's contingency is concerned. If the outcome in a given life is never absolutely predictable, it is none the less true that, whatever the outcome proves to be, it has been determined completely by cosmic and not at all by purely individual factors; and what does it matter to one that his acts are not altogether predictable so long as they are not his, or his only in an epiphenomenal, illusory sense; and so long as he

[1] The only individual which is a reality for determinism (as for Spinoza) is the universe itself; and this, if the determinism is of the naturalistic order, only in half-way fashion, since, while it credits the universe with immanent activity, it finds in it no place at all for ultimate values.

himself, so far as he exists at all, is really a spectator and not an agent in the world, even as regards the activities he calls his own?

Then, the conscious individual normally regards himself and other minds as centers of ultimate, and not merely instrumental, values. To him values are their own *raison d'être*. Nothing good is called to account by normal consciousness unless it be in view of some greater good. Again, however, determinism exposes the sawdust in the doll. Unless it be theological determinism—in which case its empirical grounds are far to seek—values, too, are linked by it into its endless causal chains, and made mere instruments of cosmic selection, happy hits of nature for ulterior purposes, shrewd devices by which she induces certain parts of herself to maintain useful types of function. Now, from this situation also, one looks in vain for relief to any contingency due merely to the infinitude of the universe.

Indeed, it seems evident that out of absolutely determined materials none but a determinist structure is likely to be built. So long as the mechanical philosopher is allowed to quarry the stone, that is, permitted without protest to reify and virtually apotheosize abstractions from the field of perceptual physics, so long are blocks of thought likely to fit into no edifice not inscribed with the name of Destiny.

WM. FORBES COOLEY.

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REVIEWS AND ABSTRACTS OF LITERATURE

Psychologie de l'Enfant et Pédagogie Expérimentale. Second edition, revised and enlarged. Dr. ED. CLAPARÈDE. Genève: Librarie Kündig. 1909. Pp. viii + 283.

This neat little volume is an attempt to furnish practical teachers with the elementary facts of mental growth together with some suggestions and illustrations of how educational questions may be investigated by the experimental method. The first 190 pages are devoted to an informal discussion of various problems of child-psychology. The author is firmly convinced that psychology, especially that of the child, is of the utmost value for the teacher. An interesting and careful historical survey of the development of child-study in all civilized countries is presented.

Next follows a discussion of problems and methods. The problems of an educational psychology are in brief: preservation of the child in health, development of the mind and body through gymnastics, the equipment of the memory and education as such; that is, the development of character. The chapter on methods simply aims to state in a general way for the teacher the different modes of approach to the subject, such as the method of introspection, observation, experimentation, etc., without attempting to go into technical details.

Under "Mental Development" he states in a popular manner various facts regarding physical growth, relation of this phase to mental growth, the importance of play and imitation in the mental development, the meaning and function of infancy, interest in education and the development of interests. Throughout he shows much familiarity with various American studies and refers to them repeatedly. The treatment is interesting and up to date, but presents nothing new to the advanced student, being, as we have said, written as an introduction for the rank and file of the public-school teachers. All the chapters are accompanied with good bibliographies from the German and English as well as from the French.

The last section of the book deals with the problem of intellectual fatigue. Here the material is more valuable for the advanced student, although the mode of presentation continues to be direct and non-technical. He offers a good *résumé* of the complexities of the problem, the factors entering into it, etc. The problem of the "fatigue coefficient" of the different subjects of study, the influence of physical work on mental fatigue, the hypothesis of a reservoir of energy, over-work, and the rest are treated quite fully. The discussion contains much that is suggestive, although the application of the doctrine of the reservoir is sometimes rather fanciful. Brief *résumés* of the ordinary fatigue tests and criticisms are included.

The main criticism to pass on the book is that it is scrappy and does not attempt to present a systematic account of mental development such as the teacher should have. Possibly, however, there is some justification for a rather full discussion of certain pertinent topics instead of a presentation of all phases in a more restricted form.

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Contributions to the Study of the Affective Processes. TAIZO NAKASHIMA. *The American Journal of Psychology,* April, 1909, Vol. XX., No. 2. Pp. 157–193.

The author's chief purpose in this investigation is to study "the mechanism of the affective judgment," and this phrase, particularly the word "mechanism," furnishes the key to the general method and attitude adopted in the getting and in the interpretation of the data. The secondary purposes were to test the usefulness of the *Reizmethode* to secure pertinent results bearing on "current affective problems," and to study affective processes by the discrimination reaction method.

In the first place the investigator appears to assume the existence of affective elements, or to work from this hypothesis. By using the method of paired comparisons and by repeating the harmonical experiments of Titchener and of Hayes he wishes to confirm directly the results in regard to affective judging incidentally got by these experimenters. Introspective analysis is the primary object, introspective records being given very fully, especially those of one subject. The data are interpreted to show that the feeling judgment is not a mediated nor resultant experience, but is as immediate as the sensory judgment; this too, contrary to the expectation

of two of his observers. Rarely is there any reason for these judgments. Something of feeling quality is intrinsic in the stimuli, although organic reactions are often mistaken for feeling itself. Tones, for example, are as directly pleasant or unpleasant as they are loud or soft. For color the same thing holds.

Using the *Reizmethode* to discover how feeling differs from sensation, no specific results were obtained and the method, for this purpose, was condemned. The author does ascribe invariable temporal precedence to sensation, opposing Wundt's view that feelings may come beforehand presaging the coming sensory experience. As incidental results the conclusion is practically stated that the pleasant-unpleasant dimension is a simple one qualitatively, the same in all sense departments, although one observer thought he found differences within this dimension. The author suggests that these differences may be sensational. Only one subject finds "mixed feelings." The author questions their existence. Likewise for the few reported cases of localization, it is suggested that on a certain hypothesis they too can be explained away.

The discrimination reaction method determines the time relations to be longer than sensible discriminations, yet that these affective processes are amenable to tests by this method.

The study shows an industrious scrupulousness, numbering, counting, calculating by highly approved methods, and quantificating even the most obvious results. It strikes the reviewer that there is something lacking, not wrong necessarily, in this method, if one judges entirely by the effect on one of a study of the exposition itself. There appears to be something mechanical and superficial about concluding for the element theory of feeling from the fact simply of the quantitatively considered immediacy of the affective judgment. In a sense the author, and Titchener also, would seem to make the feeling judgment so direct, so purely acquisitive functionally, so merely and separately a product of stimulation, as to forget, overlook, or deny its reactive, essentially active, emotive, or evaluating side altogether. Feeling, as we find by introspection when no particular theory is lurking near, genuine immediacy in the Bradleyan sense when Bradley is psychologizing,[1] is something more and something deeper than an extra item added to or in juxtaposition with the sense part of an ordinary experienced situation. It is different in kind in a deeper psychological sense than this analysis reveals. This quantitative sort of distinction above, this distinction of which time-relation tests are typical, is a literal sort of demonstration which is well enough, but which for any significance at all rests upon a profounder qualitative distinction. This latter distinction is tantalizingly assumed throughout all investigations of the type described above, which attempts laboriously to use in a new field a method originally devised for sensational data conceived by presupposition to be different.

Incidentally the reviewer is glad to note that the author (in a foot-

[1] F. H. Bradley, "On our Knowledge of Immediate Experience," *Mind*, N. S., No. 69, pp. 40–64.

note), and Titchener also, admit the existence of feelings without the pleasant-unpleasant dimension. It is interesting to speculate upon the theoretical modifications likely to follow concessions of this kind, such possibly, for example, as dimensionless feelings, since all of the other Wundtian possibilities have already been rejected by this school.

CHAS. HUGHES JOHNSTON.

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Time-Relations of the Affective Processes. TAIZO NAKASHIMA. *Psychological Review,* N. S., Vol. XVI., No. 5, September, 1909. Pp. 303–309.

The purpose is to test the time of affective arousal and the dependence of affective intensity upon the duration of the stimulus.

For colors the shortest time ranged from .84 to .98 second, and for geometrical figures, .72 to 1.08 second. The feeling arousal always followed the sensory arousal. The time sense was very definite and there were few cases of mixed feelings. The reaction time, for complex visual impressions also, proved to be somewhat longer than the cognitive reaction time, for simple colors it being considerably so. Practise shortened the time a good deal, although only a slight effect was noticeable for tones.

The conclusions from this testing of the time relations in the different sense departments, and their relations to the physiological expressions, are as follows: These judgments are as immediate as sensory judgments of psycho-physics; the time-relations, aside from being invariably longer, show relatively the same variability as those of sensory; the method of reaction is here similarly applicable; the time relations of affective to sensory processes vary in the different sensory fields, being close in the tactual and olfactory senses and remote in the color sense; and all vary with the variation of the stimulus.

The two ways of accounting for the longer time necessary for the affective arousal are sharply contrasted. One view is, that affection is a definite resultant of a sensation, sensation-idea, or idea complex, and that it is experienced after these complex processes have been consummated (his own interpretation of Münsterberg). The other view (Titchener's) is that the delayed appearance of feeling is due primarily to its lack of distinctness, reinforced by the further fact that affective judgment in complex sensory or apperceptive states is reached as quickly or more so than in the purely sensory cases. The author prefers the latter, although he does not deny affective clearness and affective attention.

The similarly possible directness of judgment in affective as well as in sensory process is stressed as a point of kinship. One feels that the author, maybe unconsciously, is nevertheless trying by some other than a direct exhaustive introspective method, or at most by a sort of nose-counting tabulation of answers, to establish qualitative likeness here. The way they objectively seem to act after measuring their tardy time of entering consciousness (as compared with sensory process), even if figures should show such absolute similarity or identity in behavior, would in no sense invalidate their absolute qualitative unlikeness.

We have certainly an introspectively clear case of qualitatively disparate and unlike mental elements or functions. It may be due to the reviewer's incorrigibly prejudiced attitude, but he can not see how the real feeling problem is illumined by these experiments, bolstered up as they are with such respectable methods; nor can the reviewer altogether agree with the interpretation of the data given, feeling, perhaps unwarrantably, that the writer too easily relegates what some of his subjects call "feelings" to his own category of "emotions" or organic or kinesthetic sensations, or that he too easily and too surely speaks of his own subjects' "alleged" qualitative differences in simple affections, or too lightly perhaps of "mixed feelings," by each one of which rejections hangs a controversy.

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La mémoire affective et l'expérimentation. TH. RIBOT. *Journal de Psychologie normale et pathologique.* Juillet–Aout, 1909, IV. Pp. 289–292.

Külpe at the recent Philosophical Congress at Heidelberg reported certain results of an experiment planned to verify Wundt's tridimensional theory of feeling. These unpublished results constitute the topic for this discussion by Ribot.

Külpe's experiment consisted in gathering under defined conditions introspective data from seven subjects. The four series were as follows: (1) The reproduction of certain sense feelings, (2) the reproduction of certain more complex sensuous situations, (3) the reproduction of certain types of complex emotions, and (4) the attempted placing in memory of the emotional experience aroused by unknown but expressive portraits.

The results were that four subjects could reproduce excitement and tension, but not pleasantness and unpleasantness. One thought that she experienced a genuine feeling image of pain or pleasure, but tended to confuse the reproduced excitement and tension with kinesthetic sensations. The other two occasionally evoked affective images, although in one case it was difficult to distinguish the feeling from its image. Both reproduced excitement and tension. All seven could reproduce pain and distinguish it from displeasure. Several thought that the affective image lost none of its original intensity. Recalled pleasantness and unpleasantness seemed to some very real, to others hallucinatory. In the fourth series the subjects did not appear to be able to refer the revived feeling to any particular former experience.

There is clearly nothing conclusive from these data. Ribot would naturally expect a greater range of individual differences than one would find in intellectual memory. Nevertheless he concludes that, very likely, as artists and designers can throw most light on the visual type of imagery, and musicians on the auditory, so we should possibly find distinguishable affective types who could possibly best clarify for us the workings of the affective recall. As is well known, Ribot believes both in the conservation

and in the reproduction of affective states. These may reproduce themselves spontaneously or we may voluntarily call them up. The former occurrence is the more common, the latter indeed being entirely unanalyzable by many persons.

The issue of the discussion, interesting as a Ribot theory, not as Külpe's interpretation of his own data, is apparently that we probably may add the affective as a memory type comparable in its specialized appearance in certain individuals to that of the motor or the visual in others. Whether we may speak in any intelligible sense of feeling imagery as we may of feeling recall the author does not discuss.

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Studies from the Psychological Laboratory of the University of Illinois. Vol. I., No. 1. STEPHEN S. COLVIN. Baltimore, Md.: The Review Publishing Co.

Two studies in animal psychology and three in the psychology of learning from the University of Illinois constitute the psychological monograph published by the *Psychological Review* in November, 1909. The volume is edited by Stephen S. Colvin, who also shares in the authorship of two of the studies—on the color perception of three dogs, a cat, and a squirrel, with C. C. Burford, and on the development of imagination in school children and the relation between ideational types and the retentivity of material appealing to various sense departments, with E. J. Myers.

The work with the animals differed from the well-known experiments of Professor Thorndike, in that the animals were kept as nearly normal and allowed as much freedom as possible. Colored paper was pasted on receptacles containing food that was accessible, paper of different colors on other boxes and pans into which the animals could not enter; certain colors were thus associated with satisfaction and other colors with failure. It was found that the animals could not only discriminate one color from another, though with considerable individual differences, but were able finally to "abstract" the colors, so that orange, for example, would be picked out by a dog immediately in whatever location or circumstances it might appear. Violet and green seemed to have less power, on the whole, of impressing these animals' minds than orange and blue.

The other study by Professor Colvin, made in collaboration with E. J. Myers, was an experiment on five hundred and twenty children in public schools and two hundred and seventy-five students of elementary psychology, with the aim of determining the predominant ideational type at different ages. Cards on which were drawn geometrical figures, nonsense characters and nonsense syllables were the material used. Later a story was read to the subjects in which auditory, motor, and visual ideas were artfully combined, in order to determine whether the visually minded would or would not remember more of the visual ideas, the motor minded of the motor ideas, and so on.

The children up to the age of ten were predominantly vizualizers.

In later childhood, auditory imagery—that is, verbal imagery—appeared. Motor imagery, it appears, plays less of a rôle than has generally been supposed. A fairly definite correspondence was established between ideational type and kind of material—visual, auditory, motor, etc.—most readily remembered by each type. Incidentally it was found that the rapid falling off of the memory curve, established by Ebbinghaus and others, did not hold when the test of memory was the amount retained instead of economy in relearning, and when sense instead of nonsense material was used.

In an interesting study on "The Analysis of the Factor of Recall in the Learning Process," Edwina E. Abbott shows that it is better in learning to pause at intervals and try to reproduce, rather than to go on unintermittently jamming the material in. The experiments were made both with nonsense syllables and words, and the subjects were five students from the psychological laboratory. The relative value of "jamming in" and recalling differs with individuals, those having strong "inner-speach" tendencies being more helped by recall than those who depend on reimaging.

Another study by the same author, "On the Analysis of Memory Consciousness in Orthography," establishes certain facts concerning the mental processes of one who learns to spell words. The author found that her four trained subjects invariably substituted visual imagery for the heard letters when the words were spelled out to them, and that the heard letters were never recalled in terms of auditory imagery. Vocalization of the letters when words are presented visually is a hindrance; vocalization of syllables, a help. In general, whatever aids visual presentation, helps along the learning process.

A paper by F. Kuhlmann, on the "Development of Instincts and Habits in Young Birds," gives the results of his observation of young birds in their nests from the time of hatching until they flew away. He recorded their rate of growth in weight, the time of appearance of various motor coordinations and the stages of the appearance of fear. A number of excellent photographs accompany the article.

ABRAM LIPSKY.

NEW YORK.

Über den Austieg der Druckempfindung. G. F. ARPS. Wundt's Psychologische Studien, Bd. IV. (1908). Pp. 431–471.

The article reports a series of experiments carried on in the Leipzig laboratory with the purpose of measuring the period of rise of a pressure sensation, in other words, of determining the relation that exists between the duration of a tactual stimulus and the intensity of the resulting pressure sensation. To this end two stimuli were presented to the subject successively, a normal stimulus of constant weight but variable duration, and a comparison stimulus of constant duration but variable weight. The rise of intensity of the sensation from the standard stimulus was measured by determining for each duration of the standard the weight of the

variable stimulus (of constant duration), which gave a sensation of subjectively equal intensity to that given by the standard.

The stimuli were two round pieces of ivory 0.5 cm. in diameter, which were set down on the dorsal side of the first phalanx of the subject's first and second fingers, respectively. The rather complicated apparatus used was mainly electrical, though the comparison stimuli were given by means of a pneumatic pressure-balance. Experiments were made both by the method of limits and by the method of constant stimuli. There was no essential difference in the results obtained by the two methods. Two different standard stimuli were used, the first 134.2 gr., the second 58.5 gr., each with variable durations of from 13 σ to 1385 σ. There were also two comparison stimuli used, of 1 sec. and 500 σ duration, respectively, each with varying weights. Four subjects were tested, although the full series of experiments, using both standard stimuli, both comparison stimuli, and all four combinations of the time-space order, was carried through with only one subject. The steps between the various comparison stimuli were sufficiently small to enable the author to plot with relative accuracy the curve of the rise in intensity of the sensation from the standard stimulus.

The most important result of the experiments was to show that the rise of the sensation seems to be to a great degree independent of the intensity of the standard stimulus—at least for the stimuli here tested. The curves for both standards (134.2 gr. and 58.5 gr.) reach their maxima at about 980 σ and sink again slightly beyond that point. For all curves there is also a secondary maximum at 380 σ, followed by a drop at about 432 σ and then a comparatively steady rise to 980 σ, the absolute maximum. These results hold for all four subjects tested and for both comparison stimuli (1 sec. and 500 σ), though the absolute values of the results differ, of course, in the different cases.

The time-error and position-error are different for the several subjects. All subjects show a tendency to overestimate the standard; for example, a comparison pressure of 156.4 gr. and 1 sec. duration is judged equal to a standard pressure of 134.2 gr. and 980 σ duration. The author explains this overestimation as due partly to the fact that the standard received a "subjective emphasis" for the subject because of the sudden starting up of the electro-magnetic apparatus. Besides this, the attention seemed to the subjects to be directed especially to the standard pressure, and this fact may have caused it to be overestimated. There are several other points of interest, including fluctuations in the attention of the subjects during stimulation, effects of contrast and assimilation both between standard and comparison pressures and between succeeding comparison pressures, and the influence which the intensity of a pressure sensation had on its apparent duration. The author does not attempt an analysis of the different factors, physiological and psychical, which are involved in the main result of these experiments, the long rise of the pressure sensation and the apparently constant character of its curve, regardless of the intensity of the sensation.

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JOURNALS AND NEW BOOKS

MIND. January, 1910. *Observations on the Case of Sally Beauchamp* (pp. 1–29): W. L. MACKENZIE. – "The most probable view is that 'Sally' was either the main mental system of a profoundly hysterical person ready to develop illusions, or delusions, out of everything, or a 'hypnotic state' unawakened, and having all the same mental qualities. When the 'real' Miss Beauchamp was formed, Sally 'goes back to where she came from'—a pathetic euphemism. Probably it is nearer the truth to say that, in going away, she 'awoke' from her 'deeper trance' and that she no longer wears into a state of super-suggestibility and delirium the new, if not the real, Miss Beauchamp. That the new Miss Beauchamp does not remember Sally's experiences as such is no proof that Sally's experiences are not playing apart in some other form or lying dormant as dispositions or traces that support the whole psycho-physical life, and may yet emerge if occasion should require." *The Present Phase of "Idealist" Philosophy* (pp. 30–45): F. C. S. SCHILLER. – Idealism overlooks "the essential selectiveness of thought." The ideal of knowledge as all-inclusive is false. Knowledge distinguishes and separates what is given together. It does not put together what is given as separate. Bradley illustrates the consequences of "verbalism" in logic, and of abstracting thought from the personality of the thinker, so that it ceases to be conceived as a human function. *On Evolutionary Empiricism* (pp. 46–62): H. S. SHELTON. – With regard to "*a priori*" principles, neither the apriorism of Kant, the empiricism of Mill, nor the postulate theory of Schiller exhausts the possible alternatives. The writer's theory admits "with the Kantians the apriority of certain ideas to the experience of the individual. With the empiricists it ultimately explains all knowledge in terms of experience. With the humanist it admits the importance of postulation and the progressive character of axioms and necessary truths. *Association and Æsthetic Perception* (pp. 63–81): J. SHAWCROSS. – The writer assumes "the generally received definition of beauty, considered as a quality of the object, namely, that it is the sensuous or material expression of an immaterial content." "Considered as a normal activity of the mind, association is indispensable to the appearance of any sensuous form as *expressive; i. e.,* unless certain associations are aroused in the mind of the beholder, no object, however beautiful in itself, can inspire in him a sentiment of beauty." *Critical Notices.* J. A. Stewart, *Plato's Doctrine of Ideas:* A. E. TAYLOR. William James (colleagues of), *Essays in Honor of William James:* HORACE M. KALLEN. W. R. Boyce Gibson, *The Problem of Logic:* S. H. MELLONE. C. and W. Stern, *Die Kindersprache:* W. H. WINCH. C. and W. Stern, *Erinnerung, Aussage und Lüge in der ersten Kindheit:* W. H. WINCH. A. E. Taylor, *Plato:* J. A. STEWART. Henry Jones, *Idealism as a Practical Creed:* J. H. MUIRHEAD. *New Books. Philosophical Periodicals. Notes.*

REVUE PHILOSOPHIQUE. February, 1910. *La logique de la contradiction (1er article)* (pp. 144–172): F. PAULHAN. – A study of the principle of contradiction in relation to intellect, feeling, and action, with special reference to its practical utility and value. *L'automatisme dans la criminalité* (pp. 144–172): MARRO. – All causes which put the mental organism in a state of inferiority tend to further the translation of motor images of criminal actions into relative movements. *L'art de l'éducation* (pp. 173–198): A. MARCERONA. – A study of the weaknesses of education and educators, with the appropriate remedies. *Analyses et comptes rendus.* Carveth Read, *The Metaphysics of Nature:* A. PENJON. Albert Steenbergen, *Henri Bergson's intuitive Philosophie:* J. BENRUBI. *Notices bibliographiques.* Anna Strong, *The Psychology of Prayer:* L. ARRÉAT. Giovanni Gentile, *Il modernismo e i rapporti tra la religione e filosofia:* L. ARRÉAT. C. Myers, *A Text-book of Experimental Psychology:* CH. LALO. H. Ebbinghaus, *Précis de Psychologie:* CH. LALO. W. James, *Précis de psychologie:* CH. LALO. B. Christiansen, *Philosophie der Kunst:* CH. LALO. H. Siebeck, *Grundfragen zur Psychologie und Aesthetik der Tonkunst:* CH. LALO. *Revue des périodiques.*

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NOTES AND NEWS

At the meeting of the English Folk-Lore Society on February 16, the Rev. J. H. Weekes read a paper on "The Congo Medicine Man and his Black and White Magic," of which the following account is from the *Athenæum* for February 26:

"Mr. Weekes said that there were some fifty different *ngangas* in the Lower Congo, there being one for every known disease and every possible emergency that could occur in native life, and that they appear to practise both black and white magic. Any person, rich or poor, man or woman, who was sufficiently artful and energetic, could become a *nganga*. Mr. Weekes went on to describe at length some of the more important of the *ngangas* and their method of practising, including one who employed thunder and lightning for inflicting injury on his client's enemy, one who had control of the rain, and one who alone could perform the ceremonies necessary to enable a man or woman to remarry. If a family suspects that one of its members is under the ban of a *nganga*, the ceremony of "marrying" the fetish into the family is gone through to restrain its eagerness for harm. The power of the fetishes belonging to the *ngangas* resided in small bundles comprising various articles, according to the nature of the power to be exercised; small portions of these bundles were placed in holes in the head or stomach of the fetish when his aid was to be invoked. Mr. Weekes laid great stress on the fact that the fetishes were not worshipped. Their powers were feared and implicitly believed in—

even to the extent of leaving a man to die when once a *nganga* had declared his approaching death—but the fetishes were never bowed down to or praised."

"LITTLE has hitherto been known of the language of the Yana tribe of Indians, who occupy part of Shasta County, in the northern region of California. This want has now been supplied by Messrs. E. Sapir and R. B. Dixon, who have contributed to the ninth volume of the Publications of the University of California a series of legends recorded from the lips of the two last survivors of those learned in the tribal traditions. One of these tales is a remarkable variant of the Prometheus type of legend, describing how Fox, Sandpiper, and Coyote stole the fire, how the world was burned, and how the thieves escaped in a basket which Spider hauled up to heaven by his thread. Another and less complete version of the tale has been published by Mr. J. Curtin in his 'Creation Myths of Primitive America.' The present collection of tales, recorded in two dialects, will preserve for the use of philologists a language which is fated before long to disappear."—*Nature,* April 7.

AT the meeting on April 4 of the Aristotelian Society, Mr. H. W. Carr read a paper on "Bergson's Theory of Instinct," of which the following abstract is from *The Athenæum* for April 16: "The philosophical aspect of the problem of instinct is concerned with the question whether instinct, as we observe it in examples such as ants and bees, is a form of knowledge, different from and comparable with intelligence, or whether it is a form of reflex action that may develop into or result from intelligence. M. Bergson holds that instinct and intelligence are two forms of psychical activity which are completely different from one another in the method of their action on inert matter; that they represent two powers which must be supposed to have lain together in the original impetus of life, and which appear to have been evolved at the expense of one another; and that they correspond to two forms of knowledge which we may distinguish in our own consciousness, viz., intelligence, which gives us our knowledge of the external world of solid matter, the subject-matter of the physical sciences, and intuition, which is a knowledge of life by life."

THE Society for Philosophical Inquiry held a memorial meeting in honor of the late Dr. William T. Harris, formerly U. S. Commissioner of Education, at the George Washington University, on Tuesday, May 3, 1910, at 4:45 with the following program: "The Genesis of the Philosopher," by Rev. Dr. J. Macbride Sterrett; "His Philosophy," Dr. Edward E. Richardson; "Dr. Harris as U. S. Commissioner of Education," Dr. Elmer Ellsworth Brown, U. S. Commissioner of Education; "Dr. Harris as Interpretor of Dante," Rev. Dr. Sewall; "Impressions of Dr. Harris as Teacher of Philosophy," Rev. Dr. U. G. Pierce.

MR. H. FOSTER ADAMS, fellow in the University of Chicago, has been appointed instructor in psychology in the University of Kansas.

PROFESSOR ARTHUR H. PIERCE, of Smith College, will assume editorial charge of the *Psychological Bulletin* in September.

MR. J. A. SMITH has just been elected Waynflete professor of Metaphysics at Oxford University, in place of Professor Case.